as to the nature of the law by which the density increases internally.[1] In the article in *Nature*, I adduced the argument on which Mr. Williams comments, as a slight corroboration of the conclusions as to the physical constitution of the planet, which have been derived from telescopic inspection, and from observation of the ellipticity of figure.

From the latter part of Mr. Williams's letter I must beg leave to dissent. If one were to describe the oceanic tides on the earth as a reeling motion of the solid earth within the sea, it would surely be a somewhat obscure description of the facts, but the reeling of the Jovian nucleus *can* only be a tidal phenomenon.[2] Now the masses of the Jovian satellites are so small, that they can only raise very small tides, except indeed on one hypothesis, of the truth of which we have no evidence, and which would not tend to explain the belts if it existed. The tide raised by a small satellite can only be large when the "free" period of oscillation of the gaseous or liquid ocean is nearly the same as the "forced" period. If this were the case with one of Jupiter's satellites, it certainly would not be so with the others. Although tides accompanied by *fluid friction* do tend to produce a longitudinal current adverse to the planetary rotation, yet no current of a millionth part of the velocity requisite for the production of the belts could possibly be occasioned by the tidal friction due to Jupiter's satellites.

For these reasons I quite dissent from Mr. Williams's explanation of the belts, and of the unequal solar rotation.

Sir William Thomson has recently pointed out, in a paper read before the Physical Society of Paris, a probable cause of the reinforcement of an atmospheric tide in the earth, due to an approximate agreement of free and forced periods of oscillation. He remarks that the semi-diurnal constituent of the barometric oscillation is nearly everywhere very much larger than was to be expected, and he shows that the sun and earth together constitute a thermodynamic engine whereby the earth's rotation is accelerated. Rough numerical calculations are given, wherefrom it appears that the amount of this acceleration may not be entirely negligeable, when we consider the degree of refinement to which modern astronomy has arrived. G. H. DARWIN

R.M.S.S. *Medway*, Southampton, Feb. 2

The Search for Coal under London

IN a recent communication to this journal I dwelt upon the importance of a systematic search being made for the Carboniferous rocks under London, by a series of borings running from north to south, and only a few miles apart; but I pointed out at the same time that much of the expenditure required for such a search might be saved by a judicious selection of sites for the first two or three borings. I then quoted the opinions of Mr. Godwin-Austen and Prof. Prestwich as to the localities at which such explorations might be undertaken with the greatest chance of success. My friend, Prof. Prestwich, has written to me expressing general agreement with the views I have put forward on the subject, but calling my attention to some other suggestions of his as to the points at which borings might be executed, with fair hopes of success. Writing in the Reports of the Coal Commission in 1870 (p. 162), Prof. Prestwich expressed himself as follows:—

"The direction of the great underground coal trough is, we think, likely to be on a line passing through North Wilts, Oxfordshire, thence across Hertfordshire, South Essex, the north-east extremity of Kent, onwards towards Calais, near to which place it is thrown out by the rise of the underlying rocks, but resumes again at Thérouanne. Or in case of the anticlinal axis taking a more southern course we should look for the coal basin or basins along a line passing from Radstock, through the Vale of Pewsey, and thence along the North Downs to Folkestone and near to Calais."

Some years later Prof. Prestwich wrote as follows:—

"In short, while there is every reason to hope that on the *south* of London we may yet find in the *Lower Greensand*, beneath the Tertiary Strata and Chalk, a source of large and valuable water-supply for metropolitan purposes, there is strong reason to believe in the probability of the discovery to the *north* of London of *Carboniferous Strata*, including possibly productive Coal-measures." . . . ("On the Range of the Lower Greensand and Palæozoic Rocks under London," by J. Prestwich. From *Quart. Journ. Geol. Soc.* for November, 1878, p. 911.)

The discovery of Upper Devonian strata, both at Turnford and at Tottenham Court Road, in both cases dipping at high angles, lends not a little support to the view that a trough of Carboniferous strata may exist between those two localities. Prof. Prestwich authorises me to state that what he would now recommend would be a boring "a mile or two north of Kentish Town, not directly north, but north-east or north-west, so as to avoid the hills—say about Edmonton on the one side, or near Edgware on the other." On the south side of London he would prefer to avoid the Lower Greensand, and would recommend a boring "just beyond its outcrop at Red Hill—somewhere between there and Horley." But he thinks that if Coal-measures were found to extend beneath the Lower Greensand, means might be found to sink through the latter, by the new appliances of which the Belgian engineers have so largely availed themselves. JOHN W. JUDD

Researches on Animals containing Chlorophyll

1. DR. BRANDT'S observations (*Sitz. d. Berlin Physiol. Gesellsch.*, Nov. 11, 1881) are upon the green bodies of *Hydra*, *Spongilla*, a fresh-water planarian, and numerous infusors. He finds that these green bodies are masses of hyaline protoplasm, containing a nucleus and a chlorophyll-granule. Sometimes two to six are present, these he considers are states of division. He regards these facts as proving that those bodies are unicellular algæ, and erects the genus *Zoochlorella*. He finds them survive isolation, and even develop starch in light. Specimens from Spongilla were taken in by infusors, but were either digested or ejected: those from a dead Hydra were, however, retained by *Paramœcium*, *Coleps*, &c. He believes that the chlorophyll never belongs to the animals, but always to algæ.

My observations deal with the yellow cells of quite different animals. I have, however, ventured the opinion that in most of the above cases, the green bodies do belong to the animals, and are not algæ, and I do not yet see sufficient reason for withdrawing that view.

2. For the yellow cells of Radiolarians and Cœlenterates (for the alga nature of which Dr. Brandt so ably argued in his former paper) he proposes the genus *Zooxanthella*. Here Dr. Brandt has doubtless priority.

3. He observes that large Radiolarian colonies show no signs of digesting foreign bodies, that these and also Spongilla can be kept best in filtered water, and that the latter will not live in a half darkened room. These facts are doubtless new.

4. Dr. Brandt concludes that the algæ maintain their hosts; that so long as the animals contain few or none, they feed in the ordinary way, but when sufficient algæ are present, they are nourished like plants. He indicates an analogy to lichens (an hypothesis which, as I also state in my paper, was first ventured by Semper), and yet points out a distinction, since in a lichen there is an association of an alga with a true parasite, here a "Symbiose" of algæ with animals accustomed to independent life, which they, however, give up, and take in no further nutriment. Thus in a morphological sense the algæ, in a physiological sense the animals are the parasites.

While welcoming Dr. Brandt's interesting paper, and while not desiring to lay too much stress on such awkward facts for his view as that *Hydra*, *Anthea*, *Velella*, &c., are quite as voracious as their congeners unprovided with chlorophyll, or that the animal may possess its chlorophyll from development, and while giving him and his predecessors all due credit for their valuable observations and theoretic insight, I must point out that (1) the *demonstration* of the truth of the view that the yellow cells of Radiolarians and Cœlenterates are algæ, (2) the development of the hypothesis of the lichenoid nature of the alliance between alga and animal into a theory of mutual interdependence, and (3) the transference of that view from the region of probable speculation into that of experimental science, remain with my paper. For it will not do to ignore, with Dr. Brandt, such weighty opposing evidence as (1) the recent direct statement of Hamann that the yellow cells of Cœlenterates are not algæ, but unicellular glands, (2) the observation of Krukenberg that *Anthea viridis* did not evolve oxygen, or (3) the failure of himself and others to prove the presence of cellulose and chlorophyll, or even to

[1] Monthly Notices of R.A.S. Dec. 1876, "On an Oversight in the Mécanique Céleste, and on the Internal Densities of the Planets."

[2] The expression "reeling" would at the first glance lead one to suppose that a diurnal tide is referred to, in which the fluid parts are carried relatively to the nucleus in the direction of the disturbing satellite, but without change of superficia form, technically a spherical harmonic deformation of the first order. But it is well known that this class of displacement must be non-existent, and therefore it must be presumed that Mr. Williams does not intend this.

confirm Haeckel's discovery of starch in Radiolarians; objections which rendered the whole matter so utterly dubious that no botanist had ever accepted it, although its importance, especially to disciples of Schwendener, is obviously great. Nor is my theory of reciprocal accommodation entitled to supersede that held by Cienkowski, and formerly by Brandt, of simple parasitism of the yellow bodies, until it has been proven (1) that animals containing algæ are actually successful beyond their fellows in the struggle for existence, (2) that the starch is rapidly consumed, and (3) that the algæ are of importance in the function of respiration, for which, again, it is necessary to show (*a*) the evolution of oxygen by the algæ, (*b*) the absorption of a large percentage by the animal, and (*c*) the displacement of the respiratory pigment by the algæ when the former is normally present.

Such being the points of Dr. Brandt's paper, and of my own as compared with it, Prof. Moseley will doubtless be the first to see that he has been mistaken in assuming, from his perusal of their abstracts, that the main points of the latter have been anticipated. I must, however, sincerely thank him for so courteously calling my attention to my ignorance of Dr. Brandt's interesting paper, which I regret having failed to review in its proper order, especially as I should then have avoided the employment of a new generic name.

It is perhaps scarcely necessary to add that although its publication has unfortunately been delayed, my paper, together with other new material, was forwarded from Naples on October 26 last to the medical faculty of this University, as trustees for the quinquennial Ellis physiology prize. PATRICK GEDDES

Botanical Laboratory, University of Edinburgh, February 11

MY friend Prof. Moseley's communication in NATURE, vol. xxv. p. 338, induces me to send you the following remarks on Mr. Patrick Geddes' interesting and important paper with the above title which appeared in NATURE, vol. xxv. p. 303, and which I should otherwise have deferred noticing until I could publish a fuller account of the whole subject.

As mentioned by Geddes, Cienkowski in 1871 clearly pointed out that the yellow cells in some Radiolaria were parasitic algæ (using this adjective in the sense of living within other animals or plants, or their tissues, *i.e.* taking up house-room). Geza Entz of Klausenburg, in 1876 (February) seems next to have called attention to the subject, but, as he mentions in the *Biologisches Centralblatt* for January 20, 1882, his paper being published in the Magyar tongue, has been hardly known or indeed accessible to the scientific world outside of his Fatherland. Touching on the views of Ehrenberg, Fred. Cohn and Stein, as to the nature of the green granules in Infusoria, Entz shows by a series of observations the chemical nature (by reagents) of some of these green bodies, and that some continue to live after the death of their hosts, growing and developing until their total evolution proved them to be forms of unicellular Algæ, such as Palmella, Gloeocystis, &c., &c, and justified the suspicion that they were "independent organisms that had forced their way into and temporarily enjoyed the hospitality of their hosts." He also shows that colourless Infusoria supplied with Palmellaceous cells rapidly become infected. In a note added to the translation of his paper in the journal above quoted, Entz mentions his later discovery of "nuclei" in these cells, and very correctly reiterates that they are but stages in the evolution of Algæ, and not species in the ordinary acceptation of that word. In 1877 (February), in ignorance of Prof. Entz's paper, I published an account of a green alga living as a guest in the fronds of other algæ, and also described its minute spores entering into and growing within the structures of Epistylis and Vaginicola, even figuring some adult forms within the lorica of *V. crystallina* and throughout the frond of the bright red *Polysiphonia urceolata*, and I ventured to suggest that these observations might throw some light on the Lichen-gonidia question. In 1881 (November) K. Brandt read a paper giving the result of a series of observations on the symbiosis (*Zusammenleben*) of algæ and animals before the Physiological Society of Berlin, an abstract of which was published in the *Biologisches Centralblatt* (December 15, 1881). Lastly comes the valuable paper of Mr. P. Geddes, in which he to a large extent confirms the observations of the previous labourers in this field of research. It may not be out of place to this very short historical sketch to add that investigations in reference to the minute alga, referred by me to Cohn's genus Chlorochytrium, now for several years continued—have enabled me to add many fresh instances of its spores finding house room in the bodies of animals, and of their accommodating themselves to the various circumstances of their "surroundings," when small they are generally greenest, and often the function of assimilation seems carried on in them to that extent as apparently to check their function of development. These observations I look forward to publishing in the *Transactions* of the Royal Irish Academy, where my earlier memoir appeared.

I make no reference here to the occurrence of chlorophyll bodies in animals apparently quite independent of the presence of vegetable cells, as brought to our notice by the researches of Sorby, Lankester, Moseley, and others. This, I take it, is not Symbiosism. E. PERCEVAL WRIGHT

On an Experimental Form of Secondary Cell

THE following description of a lead secondary cell exhibits so beautifully the part performed by the coating of red oxide of lead covering the new-made plates of Faure's accumulators, in forming them and in afterwards charging and discharging them, that I have no doubt that a trial of its experimental construction will interest those of your readers by whom improvements of the Faure's cell, in respect of retentiveness and capacity, and especially of durability, are regarded as useful objects of search, and as an important desideratum.

About four ounces of No. 5 lead-shot, cleaned and amalgamated to brightness, are placed at the bottom of a 10-oz. glass cell or beaker, so as to cover it to a depth of about half an inch, a loop of stout and clean lead wire having already been laid there flat, with its long straight part reaching vertically up the inner side of the cell to serve as a terminal for a binding screw. The straight part of the wire is lacquered in the manner usual with instrument-makers (while hot) thickly with shell-lac varnish to protect it down to the point where it turns into a loop, from acid action. The counter-plate of the cell is a thin horizontal lead one, suspended about an inch over the surface of the shots below by a strip of sufficient breadth and strength projecting from it up the inner face of the cell, to be bent over the top edge, as a carrier, and there provided with a binding screw. Before introducing it, one or two ounces of powdered minium, or red-lead, are thrown into the dilute sulphuric acid of the cell, and by a little gentle stirring, followed by very quick subsidence, this powder spreads itself evenly over the shot. When the liquid is clear, the counter-plate is introduced and the cell is coupled up to two small Grove's cells in series, so as to make the shots its anode, by connecting them with the platinum extremity of the exciting battery.

The action of the dilute sulphuric acid upon the red lead when immersed in it, besides disengagement of a little heat, and of a little contaminating carbonic acid, is partly to separate and partly to convert it into a mixture of the binoxide of lead and white lead sulphate, the two forming together a sombre red-brown powder forming a protecting layer over the stratum of metallic shot. Hydrogen is given off on the counterplate, but no oxygen gas makes its appearance at the bottom of the cell as the action of the exciting current proceeds; the nascent activity of the strongly ozonised oxygen of the pair seems to be entirely spent in oxidising the lead-sulphate already existing, and in converting it into lead-binoxide. The whole of the red powder-layer in the mean time grows uniformly and very slowly darker, until in about half-an-hour its ruddy brown colour has been completely toned down and deepened into that of the dark puce-coloured lead binoxide.

At the same time a singular action is proceeding among the metallic shot. A sort of snow of white lead-sulphate is forming on their summits and falling off them by its weight, as if showered down upon them out of the murky mass above; and there is no doubt that the presence of the minium layer serves to produce, through the medium of the lead-sulphate already there, a true corrosion of the lead, mainly conducted, as it would seem, by the acid which the process of binoxidation displaces from the superjacent sulphate.. Out of contact with the incumbent powder, the bright metallic surfaces of the shots are only slightly dimmed and tarnished with a thin film of white sulphate, and it is the most remarkable feature of the process that this white film and the white caps of sulphate formed upon the upper shots are not at all discoloured, whereas without the overlying red lead protection, the lead surfaces would be immediately sooted and beclouded over with a dark-brown coat of lead-binoxide.

The nascent energy of the oxygen is evidently suppressed, and it would seem that the current takes its way by preference